

Interactive comment on “Net ecosystem carbon exchange of a dry temperate eucalypt forest” by Nina Hinko-Najera et al.

Anonymous Referee #1

Received and published: 18 May 2016

Summary Statements:

Papers from the OzFlux network are needed to document carbon (and water) flux patterns for a host of ecosystem types that have heretofore been under-represented in global measurement networks. By sampling a drier temperate eucalyptus forest than has been studied in the past, this study brings unique observations of ecosystem scale carbon fluxes that provide a valuable expansion of the Australian and (semi-)global ecosystem flux measurement networks. The paper's major contribution and strength is to present estimates of daily, seasonal and annual NEE, GPP, and ER from multiple years of observation.

However, there are some significant problems with the methods used to estimate net ecosystem exchange from eddy covariance data, and also with the estimation of com-

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ponent gross carbon fluxes. Each concern is detailed below. Given the context of the special issue to which this paper has been submitted, it is an opportune, critical moment for ensuring high quality data and sound scientific standards for the data collection, data processing, and data analysis techniques being employed by this site's investigators and, by extension, the broader OzFlux community. Judging from what I see in this paper, and its author list that includes a number of leaders in the OzFlux network, it seems there is a need to underscore and reiterate the importance of thorough and careful implementation of EC methods. For example, the use of automated 30-minute flux calculations omits a series of important QA/QC filters and corrections that need to be applied to the high frequency (e.g. 10Hz data). This seems to have been overlooked in this work, possibly others. Implementing these post-processing steps is really not very difficult and is part of the industry standard for producing reliable datasets with the EC method. Similarly, the absence of a CO₂ profile and associated storage correction to estimation of NEE from above-canopy F_{CO₂} is quite disappointing given the stature of this forest. The EC system is also rather close to the canopy top. The footprint assessment and screening is insufficiently described. This and other problems need to be resolved or mitigated as much as possible before the paper is reconsidered for publication. They should also be brought into the routine methods used as part of OzFlux.

The paper also has some major problems with its methods of data analysis. The methods of analysis are imprecise, misleading, and even circular in a number of places. Corresponding interpretations are significantly flawed, and most of the conclusions are unsupported. Analyses do not offer insight into the functional controls or functional responses of the ecosystem to environmental conditions. Sadly the paper does not present light response curves and parameters, temperature response parameters, sensitivity to VPD, or other typical measures and metrics that one would expect to see and that would lend themselves to broader comparison to other sites and also provide a lens from which to address questions about what processes and conditions drive variations in the measured fluxes. The paper also fails to deliver on its objective of at-



tributing seasonal and interannual variability in carbon fluxes to environmental drivers. The design and structure of analyses that intend to do this are unsound. The underlying dataset may have the potential to deliver useful insights about the drivers of seasonal and interannual variability of interest but the method of analysis would need to be substantially altered. Suggestions are offered below. Though I would like to be able to recommend that this section (e.g. Figs 5 and 6, Tables 2 and 3) simply be dropped from the paper, that would leave very little in the paper beyond a simple presentation of the basic data (such as in Figures 1 through 4). Reporting rates of ecosystem fluxes from a single site without a cogent analysis of the underlying drivers or the functional responses is arguably below the bar for publication in this journal. I have no doubt that the dataset can be used to produce a more insightful and valuable contribution but unfortunately the analysis and paper does not do so in its present state.

In sum, I would suggest that the paper be completely reworked before further consideration, with attention to the core data stream and to the analyses presented and corresponding interpretations.

Main Concerns:

(1) The Study's Measurement Techniques and Data Post-Processing: The data processing is inadequate and even the core measurement technique is suspect.

(1a) Measurement of turbulent fluxes at only 3 meters above a 30 meter canopy puts these observations barely into the roughness sublayer where the near-field effects of scalar sources and sinks can have a major influence. In other words, it is unclear that these measurements can provide a reliable sample of well-mixed turbulent fluxes.

(1b) Measurement atop a ridge with sloping terrain in prevailing wind directions could significantly bias flux estimates and one cannot help but wonder what role advection fluxes may play in the very strong carbon sink that is reported for the site.

(1c) It is well known that above canopy turbulent exchange cannot be assumed to

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represent net ecosystem exchange over tall canopies because of the build-up of CO₂ during conditions of low turbulence, the subsequent ventilation of CO₂ when more turbulent conditions resume, as well as depletion of within-canopy CO₂ by uptake in the canopy but that may not be detected as prompt above-canopy flux. Therefore, estimates of NEE over tall canopies requires measurements of the vertical profile of CO₂ to estimate the storage flux in addition to turbulent above canopy flux. Unfortunately, these measurements do not appear to have been included in the present deployment. There is no mention of data post-processing techniques that are used to mitigate this problem and the effects can be quite significant. It is a problem not only for NEE but also the gross fluxes inferred from NEE separation. This, too, calls into question the validity of the data record and the findings regarding large and persistent carbon dioxide uptake at the site.

(1d) It would seem that CO₂ emissions from the diesel generator could appear as an erroneous CO₂ source from the ecosystem. The text simply states that the generator is 'remote' but how remote can it be given concerns of power loss over long cables. How this concern is mitigated should be clarified.

(1e) The explanation of footprint modeling and associated screening as well as screening based on wind direction so that "fluxes were constrained to the same forest type and dominant tree species" is inadequately described. Citing the Griebel et al. 2016 study is incomplete. The method should be explained more fully here.

(1f) Measurement of soil moisture at 5 cm depth is unlikely to represent the soil water status relevant to 25 m tall trees. Measurement at a single point is also unlikely to be adequately representative given the typically large spatial variability of soil water content.

(1g) Measurement of rainfall with a rain gage placed 1 m above the ground where trees are 25 m tall is sure to undersample the real rainfall rate above the canopy.

(1h) Data post-processing is inadequate. It is customary and important to perform post-

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processing on the high frequency (here 10Hz) data rather than to rely on automated online software to calculate half-hour averaged fluxes. Unfortunately this was not done for the present work. There does not appear to be any check for non-stationarity and detrending over the 30-minute interval. There does not appear to be any despiking of raw 1/10th of a second data, certainly not the recommended method of despiking based on a moving assessment of instantaneous data relative to the standard deviation in a time-local window of data. There cannot possibly be a co-spectral correction for issues of instrument separation or frequency attenuation of measurements, for specific conditions of stability, wind direction, turbulence intensity. There was only a simple 2D coordinate rotation rather than a full planar coordinate rotation such as with the Wilczak method. All of these elements are missing and are standard requirements for the production of reliable eddy covariance flux estimates.

(1i) NEE separation was done four ways but then the results of only a single method were selected without adequate justification. It would seem more appropriate to present results from all four methods as a source of methodological uncertainty.

(1j) A description of the method of NEE separation is needed. For example, what was the temporal window over which data were used to assess the relationship between ER (from selected Fc measurements) and environmental variables (principally temperature but also some others)?

(2) The Study's Methods of Data Analysis are Significantly Flawed.

(2a) Did analysis of the drivers of temporal variability rely on gap-filled data or only on trusted direct observations? If gap-filled data were used then all of these analyses are circular.

(2b) It is logically unsound to ask if ER relates to temperature when ER is determined based on a relationship between Fc and temperature. It is circular to use temperature to model ER over time and then to assess the importance of temperature for determining temporal variability in ER.

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(2c) The analysis that alleges to diagnose the relative importance of different environmental variables in driving seasonal variability is significantly flawed. The authors examine which environmental variable best explains variability in 30-minute 'daytime' data for each month of the year and for each flux separately. This analyzes sources of diel variation, or hourly variation, not seasonal variation. Day-time appears to be defined here as half-hours when shortwave radiation was greater than 10 W m^{-2} . Not surprisingly, sunlight turns out to be an important variable for GPP for all months of the year. This does NOT indicate that solar radiation is the dominant factor governing seasonal variability in carbon fluxes at the site. To assess that you would need to do something like use daily data, or mean daylight values for each day, and then study the full year to explore relationships to environmental variables. You would need to consider LAI dynamics as well. Of course there will likely be collinearity in these daily values, but it at least has the right time scale to answer your question of attributing seasonal variability to drivers. It is also not surprising the temperature turns out to be important for ER variability at the within-month time scale because temperature is one of the predictor variables used to model ER. Again, this is circular.

(2d) The analysis that intends to identify the relative importance of different drivers of interannual variability is also severely flawed. What the authors examine, really, is if the relative importance of drivers that determine 30-minute fluxes over the whole year differs between years. Not only does this alias diel variation into the analysis as noted above (2c) but it also conflates all seasonal variability as well. What should be done instead would be to examine daily (or monthly) anomalies relative to the across-year mean for that day (or mean monthly values). Or, you could study if the functional response of fluxes to PAR, air temperature, VPD, or soil moisture differed across years, stratifying to control for the effects of the other variables. Simultaneously you would want to study if the environmental conditions varied meaningfully across years, partly examined earlier in the paper but not linked to implications for fluxes. What might be best would be a real attribution exercise in which you use the 2010 functional response to environmental conditions to estimate what the 2011 and 2012 fluxes would have



been with that functional response surface and then compare that synthetic result to the real measured case. You could do the same for the other years. Similarly, you could use the environmental conditions of 2010 combined with the functional response of 2011 (and then 2012) to estimate the effects of any drift in the functional responses alone by, again, comparing to the real case. This would allow you to truly assess the drivers of IAV in fluxes as being due to variation in environmental conditions, variation in flux responses to environmental conditions, or both. Unfortunately what has been done is simply not testing anything meaningful about IAV in fluxes.

(2e) For the above reasons, much of the discussion section on environmental drivers of CO₂ fluxes includes misinterpretations.

(3) Discussion is Overly Confident and Conclusions are Unsupported:

(3a) L 296: Comparison to these other forest types is valuable but the narrative is overly confident here. You cannot state that leaf longevity explains the differences in GPP rates between these various forests. The findings presented in this paper do not substantiate that supposition or conjecture.

(3b) L304: Delayed spring increase in ER relative to that for GPP may be partly due to soil respiration but it could still be partly linked to temperature control on plant respiration, no? Soil respiration is not shown in the present study and so this remains supposition.

(3c) L335: This site's very large carbon sink (NEE of around -1,000 g C m⁻² y⁻¹) is surprising and noteworthy. Could it be related to secondary recovery and the site's 25 year old stand age? What is the disturbance legacy for this site?

(3d) L383: It is not much of a finding to discover that seasonality is different in a dry, warm (winter free) temperate eucalypt site compared to temperate coniferous and deciduous forest sites with a strong seasonality in climate with sustained winter freezing. Furthermore, this study did not really show the difference in seasonality explicitly. Also,

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it is stated that this alleged difference in seasonality is due to the opportunistic response of eucalypt forests. This is not supported by any analysis and it is not even clear what is meant by “opportunistic response”. A simpler explanation is that they in a different climate setting.

(3e) L 385: This study did demonstrate “that seasonal and inter-annual variability in carbon uptake were not limited by temperature but predominantly driven by radiation”. The study also did not demonstrate that “carbon loss from the forest was dominated and overall ecosystem carbon exchange dynamics were not water limited due to the high rainfall” and this sentence has a hanging statement (its first part about carbon loss).

(3f) L 387: Nothing presented in the paper quantitatively supports the statement that “temperate eucalypt forests represent a unique forest type and should be considered separately in future classifications of ecosystems...”. To demonstrate this you would need to show that these forests have a different functional response to environmental conditions than other forest types. This has not been shown and would require a synthesis analysis, not simply data from a single site. It is entirely possible that if you were to control for site and time specific conditions of LAI, PAR, VPD, Tair, and soil moisture you might find that these forests behave similarly to others. This has not been tested in the present study.

(3g) L 389: Could drop the last sentence. It doesn't seem to add much of substance and I'd argue it does not really need to be said. It sounds a bit like a proposal statement or a sales pitch.

(4) More Minor Presentation Issues.

(4a) Figure 2: it would be better to use a line chart for panel a rather than a bar chart.

(4b) Figure 2: it would be helpful to show midday VPD either in addition to or instead of mean daily VPD.

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